ings are in use in class-books in half the schools in the country.

Then, again, it may operate in some such way as this. Take Professor Coues's first edition of his 'Key to North American birds.' This author says in his preface, "Professor Baird kindly offered me the use of all the illustrations of his late review, while Professor Agassiz generously placed at my disposal the plates accompanying Mr. Allen's 'Memoir on the birds of Florida.' Several of the woodcuts have been taken from Professor Tenny's 'Manual of zoölogy,' with the author's permission; and a few others have been contributed by Messrs. Lee & Shepard. With a few exceptions, the rest of the illustrations have been drawn from nature by the author, and engraved by Mr. C. A. Walker."

Now, here is a work illustrated by 238 figures, 40 of which at least are due to the unequalled genius of Audubon and Wilson; and yet their names are not even so much as mentioned in the preface, or anywhere else in the book, in connection with its illustrations! I will say here in justice to Coues, however, that he amply corrected this in the second edition of his 'Key;' but how does it operate? Why, this way: six or seven years afterwards Prof. A. S. Packard publishes a work entitled 'Zoölogy,' wherein the chapter devoted to birds has 22 figures, at least 14 of which are reduced cuts from either Audubon or Wilson, but each one accredited as being "from Coues's 'Key.'" I hold this to be altogether wrong, and a great injustice to an author or artist naturalist, either living or dead. It is quite as easy to write fig. 465, "Summer duck — from Coues's 'Key,' after Audubon," because that perpetuates the genius of a great artist, and relieves Dr. Coues of the responsibility of having drawn the bird in question!

Foreign authors are exceedingly careful about such matters in their educational works upon biology, for they seem to appreciate the fact that to be otherwise is taking, to say the very least of it, an unfair advantage of a special worker in science, who may not care to publish 'Nature series' for the public. The very recent and admirable publications of Mivart, Claus (A. Sedgwick's translation), Wiedersheim (W. N. Parker's translation), and F. Jeffrey Bell, will bear me out in this.

On the other hand, some of our American authors fully deserve the sharpest of criticism for their carelessness in such matters, and in other cases more severe handling where it actually comes within the operation of the law.

As an example of the majority of the suggestions and views that I have just put forth, let us take a little work just gotten out by Professor Packard for the use of American youth in the schools, and a sort of first steps in zoölogy (steps surely that should be, above all others, in the right direction). I refer to the 'First lessons in zoölogy' (New York, *Holt*). In the present connection, I have nothing to do with the long list of misstatements in biology in this apparently very hastily written book, but draw upon it solely to illustrate what I have said about zoölogical figures.

Dr. Packard, in its preface, makes a very shiftless acknowledgment of some of the authorities for the illustrations, but leaves a very much larger number where he has completely ignored the artists, and finally says that eight of them were drawn by himself; trusting, I presume, that the students would choose from among the most trustworthy and best of the unacknowledged ones these eight, and accredit the author with them.

I observe among several others quite a number of the wonderfully instructive drawings of Prof. E. S. Morse, some of C. V. Riley's, two of my own (figs. 196, 197), a drawing by Coues (fig. 203), and others by Hornaday, Rymer Jones, Owen, and many others, none of which receive a single word of acknowledgment as being authority for the originals.

But now a word as to some of the drawings themselves, — illustrations that are to be presented to classes of our children, and from which they are supposed to gain or derive their *first* notions of animal forms. Take fig. 211, for example, said to be a 'head of a dove,' but of rather a raptorial variety, I should mildly suggest. Fig. 212, on the same page, looks, to my mind, far more like the claw of a young lobster than the head of a cockatoo, which it is intended to represent. There is hardly a schoolboy in America, who has ever given sufficient attention to the matter, who would not know at a glance that the 'Lobate foot of the coot' (fig. 208) is absolutely incorrect in important particulars.

As the author says in the preface that it has been 'copied by electrotypy,' I do not know the authority for the skeleton of the wild ass (fig. 251), but it certainly gives the impression that the symphysis of the pelvis is not joined, and it strikes me that a better and far safer illustration of the mammalian skeleton could have been chosen to meet the end in view. But enough; for I believe I have fairly shown that surely these are not the characters of trustworthy illustrations of zoölogical subjects to bring into the class-room. And I must believe that if any of the youthful students of this little work become naturalists by profession in after-life, and look back upon the drawings I have cited, they will agree with Professor Packard, as he expresses himself on its p. 142, and with myself, after I had seen the figures in question, that, "even after the lancelet came into being, the steps by which the genuine backboned family became recognized in animal society were painful, and only in a degree successful."

R. W. SHUFELDT.

Fort Wingate, N. Mex., Oct. 9.

## The Charleston earthquake.

I suggest an experiment which will, I think, clear up the ideas of many persons who may witness it, as to the source of the phenomena of the Charleston earthquake.

Let a large sheet of glass (thick plate-glass is perhaps best) be held in a position nearly horizontal. Place an alcohol-lamp beneath it, near enough to heat it. Long before it is hot enough to soften, it will visibly bend, and then break with noise and more or less shock. It will be violently agitated.

To apply this, suppose that dense strata of rock exist at a great depth below the earth's surface, underlying the coast region from the Alleghanies far out under the ocean; that in the course of ages portions of these sheets hundreds of feet thick, hundreds of miles wide, and perhaps a thousand miles long, have been slowly increasing in temperature, and expanding or endeavoring to expand. For a long time, and to a considerable amount of expansion over such large areas, the tendency to expand merely makes the rock denser; i.e., sets up internal strains, compressing the substance of the rock as confined — a mile square of it, fifty miles square of it — to the

actual space it has occupied for ages. This rock is like hard glass, elastic, which involves compressibility. At last the compressive stress accumulating for ages becomes too great to be borne without relief, which can come only from fracture.

The fracture, once started, extends from its initial point in lines of dislocation, as is in cold countries constantly observed in the thick ice covering lakes, and as is seen in the heated pane of glass.

But the commotion, the shock, the rending, the noises, are infinitely greater than in the case of the pane of glass or the sheet of ice. In the sudden splitting, rending, and jarring dislocation of the glass, we have the working model of the heated strata of rock. If the effect bears any proportion to the relative magnitude of the model and the rock, then we have force, stress, movement, noise enough to produce all the audible and visible effects of the Charleston earthquakes.

The sudden dislocation and displacement under Charleston may produce the local shock; the noise of the sudden splitting of the rock in place, the sound like distant cannon-shot. The long roar and grinding, like ten thousand rusty iron chariots on a rocky road, may be due to the production of a crack, which, if ten miles long, and instantaneous throughout its whole length, would yet be heard only as the sound from each foot of its length arrived at the ear of the hearer. The sound produced under foot might be heard within a few seconds; and that produced fifty or sixty thousand feet away, say ten miles, would not reach the ear till it was fifty or sixty seconds old; and, as the sound of successive portions breaking at different distances arrived, there would result a continuous and heavy roar. Such a dislocation would relieve in great measure the general, the widely diffused stress and strain. But movements would be local as well as general, and the smaller but still immense sections of our stratum of rock might continue for days and weeks to adjust themselves by smaller cracks, crushings, and dislocations, producing the lesser shocks, sounds, and roars which commonly follow the first and greatest disturbance. Such have followed that of Charleston and Summerville. In fact, the pane of thick glass breaking over the flame of an alcohol-lamp in the laboratory or on the lecture-table seems to give a working model, illustrating all the known and reported phenomena of the Charleston earthquake. The heat supposed to be observed by some in the ejection of water and mud may well have come from the sudden compression and stresses set up in the moment of dislocation. Sudden shocks, compressive stresses, and motion arrested, produce heat, as, when a fifteen-inch cast-iron ball at great velocity breaks to pieces against an iron target, its scattered fragments are all hot to the hand that gathers them. Ten miles square of hard limestone, if heated 10°, would expand three feet in length and width if free to move; heated 100°, it would expand about thirty feet each way. Here are force and movement enough to wreck a dozen Charlestons. All we need on this theory is a change of temperature not very great nor rapid.

Such changes are plainly registered in the famous three columns of Pozzuoli described by Lyell, which, having been erected above the level of the ocean, have, two or three times within the historic period, sunk below its surface, and been bored at various levels by stone-boring shell-fish (Simaceae saxophagi), and then risen again till these marks, undoubtedly made under water, are now above the water, which merely bathes the floor of the temple, and on which they still stand upright, as though never disturbed. Lyell's clear description assigns these evident changes of level to local changes of temperature in the crust of the earth below Pozzuoli. Visible motion and fracture of rocks also accompany the phenomena of 'creeping' in coal-mines. M. C. MEIGS.

Washington, D.C., Oct. 20.

## Sea-level and ocean-currents.

I have just received a letter from my friend, Capt. John Brown, son of John Brown the martyr, which I have thought would interest your readers in itself, and furnish a better illustration than I have before given of the power of wind-friction to move great bodies of water. I therefore enclose you the following copy: —

PUT-IN BAY IS., Oct. 16, 1886.

MY DEAR FRIEND, — At 11 o'clock Thursday evening, the 14th inst., I witnessed here a remarkable fact, the effect of the late tremendous wind-storm. This commenced about 7 A.M., and began to let up at 11 o'clock in the evening, or a little later. I then went down to the shore in front of my house, and found the lake lower than the average by fully six feet! This is the greatest depression from such cause I have noticed during a residence here of nearly twenty-four years. We have not, within this period, had such a high wind steadily continued for so long a time.

The captain of the steamer Chief Justice Waite, running between Toledo and the islands, reports the fall of water-level at Toledo as about eight feet.

Ever yours, JOHN BROWN, JR.

The reply of Mr. Ferrel, contained in *Science* of July 30, seems to me to obscure rather than illuminate the subject it discusses. The question before us is, not whether the wind has the power of raising the water-level on a coast, but whether wind-friction can, in the great equatorial belt and in the track of the Gulf Stream, produce the flow of water which is there observed. The striking cases of the power of wind to heap water on coasts, and to move bodily great masses of it in lakes, are only interesting and relevant as demonstrating the sufficiency of wind-friction to produce broad and rapid surface-currents. This conceded, and the case is won, because, in the lakes and open ocean, like causes produce like effects. Wind of given velocity raises in both places waves of equal height in equal times: against these waves the wind presses in the direction of its flow, with no opposing force. As a consequence, the roughened water-surface, from greatly increased friction, is moved bodily forward just as though impelled by the paddles of a revolving wheel. This surface-flow is in time communicated to underlying strata, and, if the wind continue to blow in the same direction, ultimately a large body of water will be set in motion; in other words, an ocean-current will be produced. There is no escape from this conclusion; and all that part of Mr. Ferrel's paper which relates to wind-velocities, gradients, cross-sections, etc., are irrelevant. The great truth remains, that wind-friction can produce ocean-currents. The difference in specific gravity between cold arctic and warm tropical water is undoubtedly also a *vera causa*, the only difference between Mr. Ferrel and myself being as to the relative value of these two factors. Impressed as I am with